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Alcohol policies and highway vehicle fatalities

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Abstract

This study investigates the impact of beer taxes and a variety of alcohol-control policies on motor vehicle fatality rates. Special attention is paid to omitted variables biases resulting from failing to adequately control for grassroots efforts to reduce drunk driving, the enactment of other laws which simultaneously operate to reduce highway fatalities, and the economic conditions existing at the time the legislation is passed. In the preferred models, most of the regulations have little or no impact on traffic mortality. By contrast, higher beer taxes are associated with reductions in crash deaths and this result is relatively robust across specifications.

JEL classification: I12; I18

Keywords: Alcohol; Taxes; Regulation; Vehicle fatalities

1. Alcohol policies and highway vehicle fatalities

Traffic fatalities, a major source of accident deaths at all ages and the leading cause of mortality for persons under 40, often involve alcohol. Almost half of drivers and more than 40% of passengers killed in vehicle crashes have been drinking (Zobeck et al., 1993), with still greater use of liquor among fatal accidents occurring at night. During the last 15 years, Federal and state governments have undertaken unprecedented efforts to decrease this death toll by passing

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strict regulations aimed at reducing alcohol-involved driving. For instance, the Federal Alcohol Traffic Safety Act of 1983 provided incentives for states to enact stringent drunk-driving laws and the Uniform Drinking Act of 1984 included provisions for withholding a portion of Federal highway funds from states failing to raise their minimum legal drinking age (MLDA) to 21.

By 1988, every state had mandated a 21-year-old drinking age and many had adopted other alcohol-control measures. During the 1980s, numerous states authorized police to administer roadside breath tests for alcohol, enacted administrative per se laws that require license suspension or revocation if a driver's blood alcohol content (BAC) exceeds a prespecified level, mandated minimum jail sentences or community service for driving under the influence (DUI), authorized lawsuits against alcohol servers (dram shop laws), required license sanctions for drivers refusing to submit to alcohol testing (implied consent laws), and prohibited open containers of alcohol in the passenger sections of motor vehicles. [1]

Nor does the regulatory activity show any sign of abating. For instance, by the end of 1994, 12 states had enacted illegal per se BAC levels of 0.08%, rather than using a less stringent standard (typically 0.10%), 39 had passed administrative per se laws, and 26 had established mandatory fines for the first DUI conviction (US Department of Transportation, 1995).

Ironically, the legislative action has been at least partially offset by a decline in real alcohol taxes. Despite numerous studies showing that lower liquor taxes lead to more drinking and drunk driving (see Cook and Moore, 1993a, or Grossman et al., 1993, for reviews of this literature), the Federal tax rate on beer and wine remained fixed in nominal terms between 1951 and 1991 and so fell by four-fifths, over the period, after accounting for inflation (Grossman et al., 1993). [2] Similarly, real state liquor taxes declined more than 50% between 1966 and 1993. [3]

Although traffic fatalities decreased from 51 077 in 1980 to 44 529 in 1990 (Zobeck et al., 1993), there are at least two reasons to doubt that the entire reduction is due to stricter alcohol-control regulations. First, the legislation was accompanied by substantial grassroots activities to change public attitudes towards drinking and driving. For example, Mothers Against Drunk Driving (MADD) formed its first chapter in 1981 and by 1986 had established 395 chapters (Evans et al., 1991). These community efforts may have contributed to the reduction in highway accidents. Second, the percentage of traffic deaths involving drinking did not fall significantly over the time period, suggesting that driving may have become less risky for reasons unrelated to the prevalence of alcohol (e.g. the

[1] Between 1981 and 1986, 729 drunk-driving laws were passed by states (Evans et al., 1991).

[2] The Federal tax was doubled in 1991 but has not been raised since that time and remains low, in real terms, by historical standards. The terms liquor and alcohol are used synonymously throughout this paper to indicate beer, wine, or distilled spirits.

[3] This calculation uses data on average nominal tax rates from the US Brewers' Association (1994), deflated by the all-items CPI.

establishment of mandatory seat belt laws and the increased availability of vehicle safety features such as anti-lock brakes and air bags).[4]

This investigation provides new evidence concerning the effects of alcohol policies on highway vehicle fatalities. Special attention is paid to the fragility of the estimates to plausible changes in model specifications. The predicted effects of anti-drunk-driving legislation are shown to be quite sensitive to the inclusion of covariates that are expected to affect traffic deaths but that have frequently been omitted from earlier analyses. In particular, the estimated regulatory impacts appear to be overstated when changes in public attitudes or in other legislation, which may also operate to reduce drunk driving, are not accounted for. They are further reduced by the addition of controls for state-level macroeconomic conditions. By contrast, the predicted deterrent effects of alcohol taxes are more robust to alternative specifications. Taken together, these findings suggest the limited ability of further legislative activity to reduce alcohol-involved driving but point to a potentially significant role for higher liquor taxes.

2. Previous research

Virtually all contemporary studies find a strong negative relationship between legal drinking ages and vehicle fatalities.[5] Thus, the increases in MLDAs, instituted during the 1980s, are believed to explain a substantial portion of the declining trend in traffic deaths and to have had an especially large impact on the mortality of 18 to 20 year olds. Alcohol prices and taxes are also negatively and powerfully correlated with crash fatalities, implying that the erosion in real liquor tax rates has, ceteris paribus, increased this source of mortality.

By contrast, there is less consensus on the impact of other types of alcohol-control regulations. Most available evidence suggests that administrative per se and dram shop laws reduce fatalities (Chaloupka et al., 1993; Kenkel, 1993a; Sloan et al., 1994), although some studies (e.g. Sloan et al., 1995) obtain inconclusive results. Mandatory jail sentences for DUI are found to have a deterrent effect by Kenkel (1993a), Sloan and Githens (1994), and Sloan et al. (1994), but to have no impact by Wilkinson (1987), Chaloupka et al. (1993), and Evans et al. (1991). Preliminary breath test legislation, sobriety checkpoints, anti-plea-bargaining statutes, and changes in tort liability laws reduce predicted traffic deaths in one or more studies but are not controlled for or have no effect in many others. However,

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[4] Zobeck et al. (1993) indicate that 41% of crash fatalities were alcohol related in both 1980 and 1990.

[5] Early research focused primarily on the *reductions* in drinking ages occurring in 29 states between 1970 and 1975. Later investigations have generally examined the effects of increases in MLDAs implemented during the late 1970s and 1980s. Wagenaar (1993) provides a comprehensive review of research examining the effects of raising the legal drinking age.

as discussed next, most of these estimates suffer from one or more types of omitted variables bias.

Interstate differences in vehicle mortality are likely to be influenced by disparities in difficult to observe characteristics such as road conditions, driving patterns, and social attitudes towards drinking. Many previous studies have ignored this heterogeneity, resulting in biased estimates if the unobserved factors are correlated with cross-state variations in alcohol policies. In their analysis of legal drinking ages, DuMouchel et al. (1987) compare fatality rates for 18 to 20 year olds, the group most affected by the legislation, to the corresponding rate for older drivers. This procedure is similar in spirit to the 'difference-in-differences' methods increasingly used by health and labor economists (e.g. Card and Krueger, 1994; Gruber, 1994). However, it is not applicable to other regulatory policies, which affect all age groups, and it may fail to eliminate the heterogeneity bias to the extent that MLDAs affect the vehicle mortality rates of older individuals (e.g. those who die in crashes caused by 18 to 20 year olds.).

Other researchers (e.g. Saffer and Grossman, 1987a,b; Chaloupka et al., 1993) attempt to control for the heterogeneity by including an unusually wide set of explanatory variables. Although this has the advantage of providing a more fully specified model, the measured differences are unlikely to fully account for all of the disparities between states. Even the particularly rich background information available to researchers using microdata (e.g. O'Malley and Wagenaar, 1991; Kenkel, 1993a; Sloan and Githens, 1994) is unlikely to completely eliminate the heterogeneity. The alternative, and I argue preferable, approach used in this paper is to estimate fixed-effect (FE) models. FE estimates exploit within-state variations in the regressors and outcomes and so automatically control for all time-invariant factors that differ across states. [6]

Prior investigations also typically account for only a small set of the relevant alcohol policies. For instance, DuMouchel et al. (1987) and O'Malley and Wagenaar (1991) investigate the impact of MLDAs, without controlling for liquor prices or any other DUI legislation. Even studies modeling the effects of several types of regulations simultaneously (e.g. Evans et al., 1991; Chaloupka et al., 1993; Kenkel, 1993a; Sloan et al., 1994, 1995) are unlikely to hold constant all of the important laws. Furthermore, none of the analyses controls for community activities, such as those by MADD, which have grown over time and may have reduced drunk driving both directly as well as indirectly by increasing pressure on

[6] Time-varying factors still need to be accounted for. Some researchers (e.g. Saffer and Grossman, 1987b; Chaloupka et al., 1993) question the use of FE models because of collinearity between the alcohol variables and other regressors. Multicollinearity increases the standard errors of the coefficients but does not cause bias, in contrast to methods that fail to purge the cross-state heterogeneity. The first alcohol research using fixed-effect models was Cook and Tauchen (1982, 1984). More recent examples include DuMouchel et al. (1987), Saffer and Grossman (1987b), Saffer and Chaloupka (1989), Evans et al. (1991), and Sloan et al. (1994).

states to enact anti-DUI regulations. Although these sources of omitted variables bias cannot be easily eliminated, this study provides some information on their importance.

Finally, traffic fatalities fluctuate with economic conditions. Some of the variation results from the reduction in driving that occurs during recessions. Crash deaths are also likely to be affected by cyclical changes in the type and amount of alcohol consumed and possibly in driving patterns. [7] Previous research generally fails to adequately control for the macroeconomy. Personal incomes are sometimes held constant (e.g. Saffer and Grossman, 1987a,b; Sloan and Githens, 1994; Sloan et al., 1994, 1995) but only a few studies (Wagenaar and Maybee, 1986; Saffer and Chaloupka, 1989; Chaloupka et al., 1993; Evans et al., 1991; Kenkel, 1993a) also account for differences in unemployment rates. As shown below, it is important to control for both of these factors at the same time.

3. Data

The econometric analysis uses data for the 48 contiguous states (excluding Alaska, Hawaii, and the District of Columbia) over the 1982 through 1988 time period, with information from 1975 on incorporated when describing time trends. The dependent variables are total vehicle fatality rates per capita (*TVFR*) and per mile driven (*TVMR*) as well as the per capita death rates from accidents occurring at night (*NVFR*) and for 18 to 20 year olds. The total mortality rates provide comprehensive measures of fatal highway crashes. The *NVFR*, defined to include deaths from accidents occurring between midnight and 3:59 A.M., focuses on crashes that are more likely to involve alcohol. [8] Finally, the vehicle death rate of 18 to 20 year olds concentrates on the age group most affected by changes in the legal drinking age. [9] Information on traffic mortality was obtained from the National Highway Traffic Safety Administration's Fatal Accident Reporting System (FARS). [10]

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[7] Evans and Graham (1988) provide an excellent discussion of these issues. Ruhm (1995) shows that alcohol consumption is procyclical and that the intake of distilled spirits is more sensitive to economic conditions than is that of wine or beer.

[8] Two-thirds of persons killed between 8:00 P.M. and 5:00 A.M. have BACs of 0.10% or higher (DuMouchel et al., 1987). Researchers dispute which fatality outcome should be used in studies of the effects of alcohol policies. Some (e.g. Cook and Tauchen, 1984; Saffer and Grossman, 1987b) favor comprehensive measures, such as total fatality rates, whereas others (e.g. O'Malley and Wagenaar, 1991) focus on narrower outcomes. Although a larger percentage of night-time than total deaths involve alcohol, the smaller number of crashes reduces the precision of the estimates.

[9] Preliminary analysis was also conducted on the *NVFR* of 18 to 20 year olds and for an extrapolated series estimating the percentage of dead drivers (of all ages) with BACs exceeding 0.05%. Results were similar to those for total fatalities of the corresponding age group.

[10] I thank Frank Chaloupka for providing me with data on vehicle fatality rates, beer taxes, and state alcohol regulations.

The minimum legal drinking age refers to purchases of beer with an alcohol content greater than 3.2%. Five other alcohol regulations are modeled as dichotomous variables. Preliminary breath test laws (*BREATH*) indicate whether the state has authorized a breath test as establishing probable cause for DUI. Dram shop legislation (*DRAM*) refers to case law or statutes allowing those injured by intoxicated persons to bring litigation against the alcohol server. Administrative per se regulations (*PER SE*) show if the state licensing agency is required to suspend or revoke the individual's license after a DUI arrest. Implied consent legislation (*CONSENT*) indicates whether the state has instituted mandatory license suspension for individuals refusing to submit to alcohol testing. Finally, *JAIL* refers to legislation mandating jail or community service for the first DUI conviction. These regulations are not assumed to fully capture government efforts to reduce alcohol-involved driving. Instead, they are chosen to provide a reasonably representative selection of the types of policies considered in previous research. Data on all these variables were obtained from various issues of the National Highway Traffic Safety Administration's *Digest of State Alcohol-Highway Safety Laws.* [11]

The real tax rate on 24 12-oz. containers of beer is controlled for using data from the *Brewer's Almanac*, which is published annually by the US Brewers' Association. [12] Information on taxes, rather than prices, is used for two reasons. First, the tax rates are directly set by policy-makers, whereas prices are not. Second, prices result from the interaction of supply and demand, whereas taxes are relatively unaffected by the latter. [13] Thus, whereas beer taxes and alcohol consumption are expected to be inversely related, quantities and prices will be positively correlated if demand shocks dominate. [14]

The econometric models also hold constant the fraction of the state population residing in 'dry' counties (*DRY*), the proportion of drivers between 15 and 24 years old (*YOUNG*), and in the per capita fatality equations, the average number of miles driven by persons aged 16 and over (*VEHICLE MILES*). Each of these factors may influence traffic mortality and vary over time. Data on *DRY* are from the *Brewer's Almanac*; those for *YOUNG* and *VEHICLE MILES* are from various issues of the Federal Highway Administration publication *Highway Statistics*.

[11] Information on changes in MLDAs occurring during the 1970s is from Wagenaar (1981/2).

[12] Weighted averages are used to reflect changes in beer taxes and MLDAs occurring during the middle of calendar years.

[13] Beer taxes are highly correlated with tax rates on wine and distilled spirits and, hence, provide a good proxy of overall alcohol tax rates.

[14] Alcohol taxes could be endogenous, if policy-makers use them as part of a coordinated effort to reduce drunk driving. There is little indication that this occurred during the period of analysis. Instead, real tax rates declined steadily over time, despite an escalation of anti-DUI regulation. Further discussion of this issue is reserved for Section 4.2.

Per capita income and unemployment rates are used as proxies for macroeconomic conditions. Unemployment is expected to be negatively correlated with traffic fatalities, since total alcohol consumption and the proportion of drinking occurring in bars and restaurants is likely to fall during downturns. Personal income is predicted to be positively related to vehicle deaths, if drinking and risky driving are normal goods. A consistent (unpublished) series on unemployment rates for the non-institutionalized civilian population aged 16 and over was provided to me by the US Bureau of Labor Statistics. Data on per capita incomes were obtained from US Department of Commerce (1989, 1990).

Variable definitions and means are presented in Table 1 and are self-explanatory. Fig. 1 displays trends in vehicle fatalities, drinking ages, and beer taxes over the 1975–1988 time period. Traffic deaths peaked between 1978 and 1980, depending on the type of mortality, declined dramatically through the early 1980s, and then leveled off. MLDAs were unchanged between 1975 and 1978 and then rose rapidly during the next decade (increasing from 19.6 to 21.0 years old

Table 1
Description and sample means of variables used in analysis

Variable description and source	Mean
Outcome variables (source: *Fatal Accident Reporting System*)	
Vehicle fatality rates per 10000 persons	
Total vehicle fatality rate	1.88
Night-time vehicle fatality rate (12:00–3:59 A.M)	0.37
Total vehicle fatality rate: 18–20 year olds	4.29
Vehicle fatality rate per 100000000 miles driven	2.50
Explanatory variables	
Tax (in $1988) on 24 12 oz. containers of beer (source: *Brewer's Almanac*)	$0.49
Minimum legal drinking age in years (source: Wagenaar, 1981/2; *A Digest of State-Alcohol Safety Related Legislation*)	20.5
Percentage living in 'dry' counties (source: *Brewer's Almanac*)	4.52
Percentage of licensed drivers aged 24 and younger (source: *Highway Statistics*)	18.0
Average miles driven per person aged 16 and over (source: *Highway Statistics*)	7525
Civilian unemployment rate (%) (source: unpublished Bureau of Labour Statistics data)	7.55
Per capita income in $1987 (source: US Department of Commerce, 1989, 1990)	$14725
Driving-related legislation (source: *A Digest of State-Alcohol Safety Related Legislation*)	
BREATH: police authorized to administer pre-arrest breath test for alcohol (%)	42.7
DRAM: statute or case law authorizing parties injured by intoxicated driver to file a lawsuit against the alcohol server (%)	67.4
PER SE: state licensing agency required to suspend or revoke driver's license after arrest for DUI (%)	22.4
CONSENT: law requiring licence sanction for refusing to submit to alcohol testing (%)	62.6
JAIL: law requiring jail sentence or community service for first DUI conviction (%)	22.5

Note: data are for the 48 contiguous states over the period 1982–1988 ($n = 336$). Means are weighted by the non-institutionalized population aged 16 and over in each state.

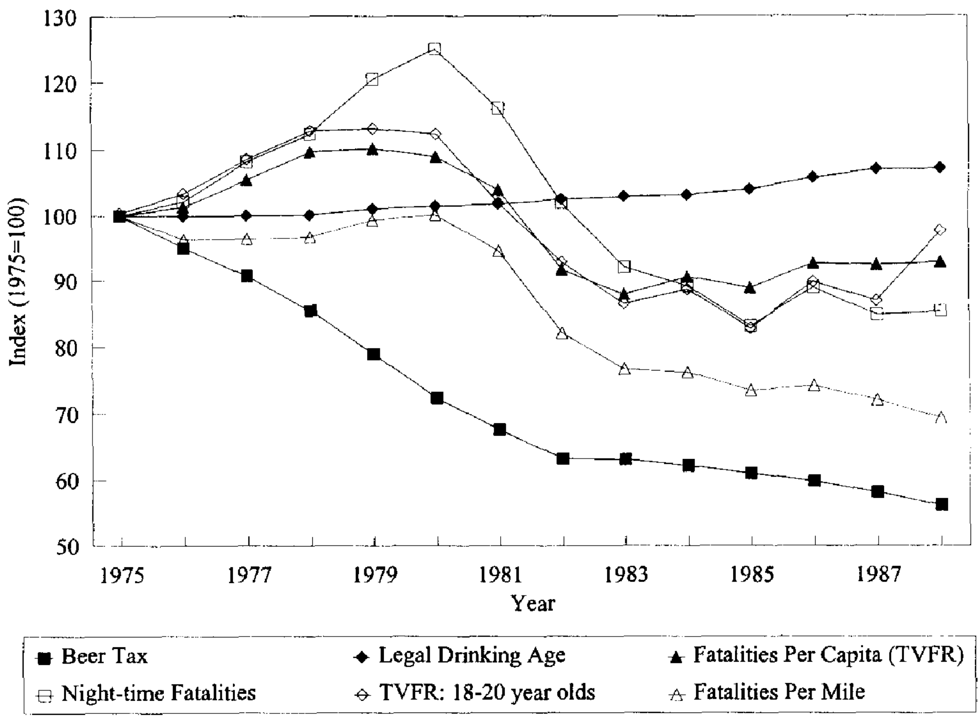

Fig. 1. Time trends in selected variables, 1975–1988.

between 1978 and 1988), particularly after passage of the Uniform Drinking Act of 1984. Conversely, substantial reductions in real beer taxes occurred during the high inflation years of the late 1970s and early 1980s (the average tax rate declined from $0.81 to $0.55 per case in $1988 between 1975 and 1981), followed by somewhat smaller decreases subsequently (to $0.46/case in 1988).[15] Finally, between 1982 and 1988, 18 states added administrative per se statutes, 8 enacted dram shop laws, and 6, 5, and 4, respectively, passed preliminary breath test, mandatory jail or community service, and implied consent legislation.

4. Econometric estimates

Most of the econometric estimates below come from fixed-effect models of the form:

$$V_{it} = \alpha_t + X_{it}\beta + Z_{it}\gamma + S_i + \mu_{it}. \tag{1}$$

where V_{it} is a transformation of the vehicle fatality rate for state i at time t, X is a

[15] *Nominal* beer taxes were unchanged throughout the entire 1975–1988 (1982–1988) period in 22 (30) states, resulting in a 52.7% (19.3%) decline in real tax rates in these locations. Conversely, real beer taxes rose in only 4 (8) states between 1975 and 1988 (1982 and 1988).

vector of alcohol policy variables, $\mathbf{Z}$ includes other (time-varying) covariates, $\boldsymbol{\alpha}$ is a time-specific intercept (a vector of dummy variables), $\mathbf{S}$ a state fixed effect (a vector of state dummy variables), μ is the error term, and the data cover the period 1982 through 1988. Equations which exclude the state or time effects are also sometimes estimated.

Since vehicle mortality is a rate, restricted between zero and one, the equations are estimated as grouped data logit models. Thus, if v_{it} is the traffic fatality rate, the regressand is $V_{it} = \ln[v_{it}(1 - v_{it})^{-1}]$. The error term is heteroscedastic, with variance $[v_{it}(1 - v_{it})n_{it}]^{-1}$, for state population n, so the estimates are by weighted least squares, with cell weights $[v_{it}(1 - v_{it})n_{it}]^{1/2}$.

4.1. Total vehicle fatalities

Table 2 summarizes the results of eight specifications of the total vehicle fatality per capita equation. In addition to the regressors displayed, *YOUNG*, *DRY*, *VEHICLE MILES*, and a vector of year effects are controlled for. [16] State dummy variables are omitted from column (a) but included in specifications (b) through (g). The only alcohol variables controlled for in columns (a) and (b) are beer taxes and drinking ages. Models (c) through (g) add dichotomous variables for the five DUI regulations (*BREATH*, *DRAM*, *PER SE*, *CONSENT*, and *JAIL*) while columns (d) through (g) hold constant one or more measure of economic conditions. Model (f) is restricted to time and year effects and those covariates which are statistically significant, at the 0.1 level, in the most fully specified equation, column (e). Specification (g) is the same as model (e), except that the year dummy variables are omitted.

The advantage of controlling for fixed effects is illustrated by comparing columns (a) and (b). When state dummy variables are excluded, specification (a), the beer tax coefficient has the ‘wrong’ sign – taxes are positively and significantly related to predicted traffic fatalities. By contrast, the tax parameter has the expected negative value when state fixed effects are included, model (b). The sensitivity of the estimated tax effect to the inclusion of state-specific intercepts was further tested for using a variety of alternative specifications of the *TVFR* equation and also in models examining the per capita death rates of 18 to 20 year olds. These results (which are not displayed) confirm that the tax coefficient is frequently positive and highly sensitive to the choice of regressors when fixed effects are omitted but significantly negative and relatively robust to these changes in the FE estimates. [17]

[16] Fatalities are positively and significantly related to average vehicle miles and the percentage of the population living in dry counties in most specifications. Coefficients on the proportion of young drivers are also usually positive but frequently statistically insignificant.

[17] The tax coefficients in total fatality equations equivalent to specifications (a), (c), and (e), except without fixed effects, are 0.1245, 0.1113, and 0.0508. The corresponding parameter estimates for equations examining the traffic deaths of 18 to 20 year olds are 0.0005, −0.0159, and −0.0870.

Table 2
Econometric estimates of the determinants of total vehicle fatalities per capita

Regressor	Specification						
	(a)	(b)	(c)	(d)	(e)	(f)	(g)
Beer tax	0.1245	−0.3235	−0.3406	−0.2717	−0.2414	−0.2555	−0.2484
	(5.88)	(3.99)	(4.32)	(3.77)	(3.78)	(4.09)	(3.51)
	[0.06]	[0.15]	[0.16]	[0.13]	[0.11]	[0.12]	[0.12]
Minimum legal drinking age	−0.0238	−0.0219	−0.0129	−0.0145	−0.0062		−0.0136
	(1.81)	(2.78)	(1.61)	(1.99)	(0.96)		(1.99)
	[0.49]	[0.45]	[0.27]	[0.30]	[0.13]		[0.28]
Preliminary breath test law			0.0056	0.0123	0.0249		-4.1×10^{-4}
			(0.24)	(0.59)	(1.33)		(0.02)
Dram shop law			−0.1088	−0.0608	−0.0380	−0.0426	−0.0706
			(4.93)	(2.90)	(2.03)	(2.39)	(3.48)
Administrative per se law			−0.0224	−0.0019	−0.0010		−0.0295
			(1.13)	(0.11)	(0.06)		(1.69)
Implied consent law			−0.0935	−0.0398	0.0102		−0.0360
			(1.96)	(0.91)	(0.26)		(0.83)
Mandatory jail/ community service			0.0164	0.0016	0.0097		−0.0023
			(0.59)	(0.06)	(0.43)		(0.09)
Personal income (in $1000s)				0.710	0.0349	0.0344	0.0144
				(7.74)	(3.83)	(3.93)	(1.62)
				[1.05]	[0.51]	[0.51]	[0.21]
Unemployment rate					−0.336	−0.0338	−0.0210
					(8.72)	(8.98)	(5.54)
					[0.25]	[0.25]	[0.16]
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	No
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes

Note: table shows results of the grouped data logit model $\ln[v_i/(1-v_i)] = X_i\beta + Z_i\gamma + \mu_i$, estimated by weighted least squares, where v is the total vehicle fatality rate per person. Pooled data are used for the 48 contiguous states over the period 1982 through 1988 ($n = 336$). The equations also control for the percentage of the state population living in 'dry counties', the average number of vehicle miles per person aged 16 and over, and the percentage of drivers who are 'young' (15–24 years old). Absolute values of t statistics are shown in parentheses. Absolute values of estimated elasticities are shown in brackets. Elasticities are calculated as $\varepsilon_j^* = (1-\bar{v})\hat{\beta}_j\bar{X}_j$, where $\hat{\beta}_j$ is the logit coefficient of the jth regressor and $\bar{X}_j$ and $\bar{v}$ are the sample means of the explanatory variable and the motor vehicle fatality rate respectively. Specification (f) includes only those regressors that were statistically significant, in model (e), at the 0.1 level.

The importance of accounting for simultaneous changes in different types of DUI legislation is revealed by comparing results for specifications (b) and (c). Whereas the beer tax coefficient is virtually unaffected by the addition of the five new alcohol regulations, the predicted MLDA elasticity declines in absolute value by more than 40% (falling from −0.45 to −0.27). This suggests that previous investigations may have overstated the benefits of raising the minimum legal drinking age, to the extent they do not account for the effects of other measures designed to deter drunk driving that were being enacted or strengthened at the same time.

Columns (d) and (e) demonstrate the need to also control for macroeconomic conditions.

The predicted beer tax elasticity falls by 30% (from −0.16 to −0.11), with the addition of the unemployment and income regressors, while the corresponding decrease in the *MLDA* elasticity exceeds 50% (from −0.27 to −0.13) and no longer differs from zero at conventional significance levels (compare columns (c) and (e)).[18] Notice that *both* per capita income and unemployment rates are useful covariates. Controlling for income alone actually slightly increases the estimated drinking age effect (see column (d)), whereas holding unemployment constant as well dramatically reduces it, specification (e). This occurs because fatalities are procyclical and rates of joblessness increased over time in states raising drinking ages during the middle 1980s, relative to those with a 21-year-old MLDA throughout the period.[19] As anticipated, incomes (unemployment rates) are positively (negatively) associated with traffic deaths.

Among the other types of drunk-driving legislation, only dram shop laws are negatively and significantly correlated with traffic deaths in the most fully specified model, column (e). Even for this class of regulations, however, the parameter estimate is only around one-third as large as when macroeconomic conditions are ignored (compare specifications (c) and (e)).[20] The coefficients on *BREATH*, *PER SE*, and *JAIL* are never significant, at conventional levels, and are positive more often than negative. The parameter estimate on *CONSENT* is negative and significant in specification (c) but becomes insignificant in models (d) and (e).[21]

[18] Since taxes accounted for around one-eighth of total beer prices in 1988, a tax elasticity of −0.11 translates into a price elasticity of approximately −0.9. This is at the high end of the estimated effects obtained in previous analyses of aggregate data but is considerably smaller than the elasticities frequently derived from individual data (see Leung and Phelps, 1993). These estimates imply that increasing the real beer tax from its 1988 level of $0.454/case to the 1975 level of $0.808/case (in $1988) would reduce per capita vehicle fatalities by 7.9%. This and all subsequent calculations of policy effects assume that the regressors, other than the policy variables of interest, are set equal to their sample means.

[19] The population-weighted average unemployment rates of the former group of states fell from 8.8% in 1982 to 5.4% in 1988, versus a larger reduction from 10.5 to 5.7% for the latter group.

[20] The imposition of dram shop laws in every state reduces predicted fatalities by 3.7% in specification (e), versus 10.3% in model (c).

[21] Generally similar results were obtained in models without state fixed effects. Important differences include the aforementioned instability of beer tax coefficients, significant positive coefficients on *JAIL*, and negative parameter estimates for personal incomes. The last two results suggest that states pass mandatory jail/community service laws when concerned about high rates of traffic fatalities and that death rates are high in relatively poor states, possibly due to inferior road conditions or vehicle maintenance.

Specification (f) deletes those regressors which were not statistically insignificant, at the 0.1 level, in model (e).[22] These exclusions have little impact on the remaining parameters. The coefficients on personal income and unemployment rates are essentially unchanged while those on beer taxes and dram shop legislation rise, in absolute value, by 6 and 12%.

4.2. *Unmeasured factors*

The alcohol regulations controlled for in Table 2 could be correlated with related policies (e.g. anti-plea-bargaining statutes, mandatory fines, or sobriety checkpoints), resulting in possible omitted variables bias. An additional potentially important source of confounding factors is the increased intensity of community efforts to change public attitudes towards drinking and driving. As discussed, the influence of groups such as Mothers Against Drunk Driving, which lobby for strict regulatory policies but also attempt to reduce drunk driving in other ways (e.g. designated driver programs), grew substantially during the period investigated. To the extent that the legislation proxies these grassroots activities, the econometric estimates will overstate the impact of the laws. Without acquiring detailed data on the local efforts, this source of spurious correlation cannot be eliminated. However, some indication of the size of the resulting biases can be obtained.

State and national legislatures have typically viewed alcohol taxes as a source of budget revenues, rather than as a determinant of drinking behavior. Therefore, beer taxes are unlikely to be highly correlated with changes in DUI legislation or community efforts to decrease drunk driving. Indeed, the tax rate declined in real terms throughout the period of intensive grassroots and regulatory activity. By contrast, the drunk-driving laws analyzed were changing at approximately the same time as other (unmeasured) regulatory policies, presumably partially as the result of community efforts to change patterns of alcohol use. Thus, states adopting the most stringent DUI legislation are likely to have been those with the strongest grassroots movements. Legal drinking ages represent a middle ground. They were probably powerfully affected by local activities prior to passage of the Federal Uniform Drinking Act of 1984. Subsequently, states were required to institute a 21-year-old MLDA, or else lose a portion of their Federal highway funds. Hence it is likely that the last states to raise drinking ages may have been those with relatively weak community sentiments against drunk driving.

These arguments suggest that the omitted variables biases will be most severe when estimating the effects of anti-DUI legislation but less problematic when considering alcohol taxes. Evidence on the effects of confounding factors for which information is available supports this expectation. The predicted MLDA elasticity is dramatically reduced by the addition of the five types of DUI

[22] The full set of time and year dummy variables is retained.

legislation, whereas the estimated beer tax effect actually increases slightly (compare specifications (b) and (c) of Table 2). The tax estimates are also less sensitive to the inclusion of the macroeconomic variables than are those for the alcohol regulations. For example, the beer tax coefficient falls 29% between specification (c) and (e), as compared to decreases of 52%, 65%, and 96% for *MLDA*, *DRAM*, and *PER SE* respectively.[23]

Further information is provided by considering equations which exclude the vector of year effects, model (g). When year dummy variables are included, specification (e), the growth of national grassroots or (unmeasured) legislative efforts to reduce drunk driving will be captured by a declining time trend in the individual year intercepts.[24] Conversely, in column (g), these effects will be incorporated into the parameter estimates for the five regulations controlled for, to the extent that the latter are correlated with the changes in community activities or in other types of legislation. The impact of local anti-drunk-driving campaigns that exceed or fall short of national efforts will not be accounted for by this comparison.

If liquor taxes are set independently of the non-legislative activities, whereas the enactment of DUI regulations is not, we expect the exclusion of time effects to result in a smaller increase in the estimated beer tax parameters than of those related to alcohol laws. A comparison of specifications (e) and (g) verifies that this occurs. For example, the *MLDA* and *PER SE* coefficients are more than twice as large in the latter case as in the former and the already substantial coefficient on *DRAM* increases by 86%. By contrast, the absolute value of the beer tax elasticity rises less than 3%.[25]

4.3. Vehicle fatalities per mile driven

The vehicle fatality equations were next re-estimated with traffic deaths per mile driven as the dependent variable. These results, which are summarized in Table 3, are similar to those for per capita mortality rates. The beer tax coefficient

[23] Conversely, additional specifications (not shown) indicate that the income and unemployment coefficients are virtually unaffected by the inclusion of anti-DUI legislation. In combination, these results suggest that the estimated regulatory effects partially proxy the impact of macroeconomic conditions, when the latter are not controlled for, whereas the reverse is not true.

[24] Ceteris paribus, predicted fatality rates, in model (e), are 19% lower in 1988 than in 1982.

[25] Other researchers have used simultaneous equation techniques to test the exogeneity of alcohol policies. For example, Saffer and Grossman (1987a) estimate a two-equation model where the vehicle mortality rate (drinking age) is the dependent (explanatory) variable in one equation and a regressor (outcome) in the second. The other covariates are the same in both equations and the model is identified by the assumed nonlinear specification – the mortality (drinking age) equation is estimated as a logit (ordered probit) model. I have not used this strategy because of the difficulty in identifying the model by plausible exclusion restrictions, rather than relying on possibly arbitrary functional form assumptions.

Table 3
Econometric estimates of the determinants of fatalities per mile driven

Regressor	Specification						
	(a)	(b)	(c)	(d)	(e)	(f)	(g)
Beer tax	0.1082	−0.2875	−0.2960	−0.2405	−0.2123	−0.1983	−0.1864
	(5.71)	(3.22)	(3.37)	(3.84)	(3.67)	(2.52)	(2.10)
	[0.05]	[0.14]	[0.14]	[0.11]	[0.10]	[0.09]	[0.09]
Minimum legal drinking age	−0.0291	−0.0191	−0.0122	−0.0133	−0.0059		−0.0207
	(2.36)	(2.19)	(1.35)	(1.54)	(0.73)		(2.40)
	[0.60]	[0.39]	[0.25]	[0.27]	[0.12]		[0.42]
Preliminary breath test law			0.0174	0.0229	0.0341		−0.0105
			(0.67)	(0.92)	(1.46)		(0.41)
Dram shop law			−0.0906	−0.0533	−0.0332		−0.0835
			(3.68)	(2.16)	(1.42)		(3.28)
Administrative per se law			−0.0519	−0.0367	−0.0363	−0.0317	−0.0757
			(2.36)	(1.72)	(1.82)	(1.66)	(3.47)
Implied consent law			−0.0875	−0.0456	−0.0012		−0.0575
			(1.64)	(0.88)	(0.02)		(1.05)
Mandatory jail/ community service			0.0029	−0.0088	−0.0018		−0.0052
			(0.10)	(0.29)	(0.06)		(0.17)
Personal income (in $1000s)				0.0549	0.0228	0.0283	0.0150
				(5.09)	(2.00)	(2.59)	(1.36)
				[0.81]	[0.33]	[0.42]	[0.22]
Unemployment rate					−0.0298	−0.0321	−0.0141
					(6.18)	(6.94)	(2.98)
					[0.23]	[0.24]	[0.11]
Time effects	Yes	Yes	Yes	Yes	Yes	Yes	No
State fixed effects	No	Yes	Yes	Yes	Yes	Yes	Yes

Note: see note on Table 2. Table shows results of weighted least squares estimates of the grouped data logit model $\ln[v_i/(1-v_i)] = \boldsymbol{X}_i\beta + \boldsymbol{Z}_i\gamma + \mu_i$, where v is the vehicle fatality rate per 10000 miles driven. The equations also control for the percentage of the state population living in 'dry counties' and the percentage of drivers who are 'young' (15–24 years old).

again switches from positive to negative with the addition of state fixed effects and declines moderately, in absolute value, when the macroeconomic variables are controlled for. The MLDA impact also falls as covariates are added and becomes statistically significant in the most fully specified model, column (e). Finally, as with deaths per capita, fatalities per mile are positively (negatively) related to per capita incomes (unemployment rates). The size of the estimated effects is typically slightly smaller than in the per capita fatality equations. For instance, the elasticities on beer taxes, MLDAs, personal incomes, and unemployment rates are −0.10, −0.12, 0.33, and −0.23 in column (e) of Table 3, as compared to −0.11, −0.13, 0.51, and −0.25 for the same specification in Table 2.

There are two important changes in the predicted effects of the regulatory policies. First, the dram shop coefficient is 12 to 17% smaller for deaths per mile than for fatalities per capita and is statistically insignificant in specification (e).

Table 4
Fixed-effect estimates of various types of per capita vehicle fatality rates

Regressor	Night-time fatalities per capita				Fatalities per capita of 18–20 year olds			
	(b)	(c)	(e)	(g)	(b)	(c)	(e)	(g)
Beer tax	−0.4364	−0.4489	−0.3808	−0.4397	−0.4231	−0.4258	−0.3462	−0.4398
	(3.41)	(3.49)	(3.05)	(3.15)	(3.12)	(3.11)	(2.62)	(3.04)
	[0.21]	[0.21]	[0.18]	[0.21]	[0.20]	[0.20]	[0.17]	[0.21]
Minimum legal drinking age	−0.0271	−0.0145	−0.0063	−0.0146	−0.0552	−0.0514	−0.0441	−0.0412
	(2.31)	(1.16)	(0.52)	(1.12)	(4.27)	(3.74)	(3.33)	(3.00)
	[0.56]	[0.30]	[0.13]	[0.30]	[1.13]	[1.05]	[0.90]	[0.84]
Preliminary breath test law		0.0289	0.0431	0.0117		0.0331	0.0522	0.0538
		(1.16)	(1.24)	(0.31)		(0.86)	(1.41)	(1.37)
Dram shop law		−0.1141	−0.0749	−0.1218		−0.0589	−0.0082	−0.0182
		(3.34)	(2.15)	(3.20)		(1.58)	(0.22)	(0.45)
Administrative per se law		−0.0338	−0.0262	−0.0791		−0.0676	−0.0556	−0.0929
		(1.12)	(0.90)	(2.45)		(2.06)	(1.74)	(2.72)
Implied consent law		−0.0356	0.0349	−0.0515		−0.0761	0.0122	−0.0418
		(0.49)	(0.49)	(0.64)		(0.98)	(0.16)	(0.50)
Mandatory jail/ community service		−0.0006	0.0024	−0.0339		0.0112	0.0099	−0.0407
		(0.02)	(0.06)	(0.77)		(0.24)	(0.22)	(0.86)
Macroeconomic variables	No	No	Yes	Yes	No	No	Yes	Yes
Time effects	Yes	Yes	Yes	No	Yes	Yes	Yes	No

Note: see note on Table 2. All equations include state dummy variables and covariates for average vehicle miles, the percentage of young drivers, and the percentage of the population living in 'dry' counties. Specifications (b), (c), and (e) also include a vector of year dummy variables and models (e) and (g) control for personal incomes and state unemployment rates. Night-time fatalities are defined as deaths from accidents occurring between 12:00 and 3:59 A.M.

Second, the deterrent effect of administrative per se laws is substantially larger and is significant at the 0.1 level in all specifications. [26]

The exclusion of year effects, column (g), once again substantially raise the predicted deterrent effects of the regulatory variables, while having little impact on the estimated beer tax elasticities. This provides a further indication that alcohol-control policies were being enacted during a period of growth in grassroots activities to deter drunk driving. Conversely, beer taxes do not appear to be correlated with these non-legislative efforts.

4.4. Night-time and youth fatalities

Table 4 summarizes the results of FE estimates for night-time deaths per capita and fatal accidents involving 18 to 20 year olds. The specifications correspond to

[26] The passage of dram shop laws in every state is predicted to reduce vehicle fatalities per capita and per mile by 3.7% and 3.3%, respectively, in specification (e). The enactment of administrative per se legislation reduces the corresponding death rates by 0.1% and 3.6%.

those in Table 2. Results for the *NVFR* are similar to those for all traffic deaths. In particular, the estimated *MLDA* elasticity declines dramatically in absolute value (falling from −0.56 to −0.30) when the five types of DUI legislation are added, specification (c), and becomes a statistically insignificant −0.13 when macroeconomic conditions are also held constant, specification (e). The beer tax elasticities are again fairly robust to the addition of covariates and are of somewhat larger size than for the *TVFR* (−0.18 vs. −0.11 in specification (e)). Dram shop laws also have a stronger expected negative impact on the night-time than total deaths. [27] Lastly, the predicted deterrent effects of the alcohol regulations again rise substantially with the exclusion of year dummy variables, specification (g), whereas a much smaller increase is observed for beer taxes.

Four important differences are uncovered when considering deaths of 18 to 20 year olds. First, the drinking age has a substantial and significant negative predicted impact in all specifications (e.g. the *MLDA* elasticity is −0.90, in model (e), versus −0.13 for the *TVFR*) and the parameter estimate is much more robust to model changes than is that for total traffic deaths. [28] Second, the deterrent effect of beer taxes is also somewhat stronger (the elasticity in specification (e) is −0.17, as compared to −0.11 for the *TVFR*). Third, dram shop laws have little or no impact on youth fatalities, probably because 18 to 20 year olds do relatively little of their drinking in bars or restaurants. [29] Fourth, administrative per se laws have a stronger predicted deterrent influence for young adults than for the full sample. [30]

5. Conclusion and implications

Although many states have recently enacted or strengthened regulations designed to deter drunk driving, previous research supplies limited and often

[27] Universal enactment of dram shop laws is predicted, by specification (e), to reduce night-time fatalities by 7.2% and all traffic deaths by 3.7%.

[28] The MLDA effect appears to be largely restricted to persons below the age of 21. This can be seen by noting that 18–20 year olds accounted for 11% of vehicle fatalities and 15–20 year olds for almost 18% of traffic deaths, during the 1982–1988 period, while the estimated elasticity is 14% as large for total traffic fatalities as for deaths involving 18–20 year olds.

[29] Enactment of dram shop legislation in every state reduces the predicted death rate of 18–20 year olds by less than 1%.

[30] The passage of administrative per se legislation in all states decreases the predicted per capita death rate of 18 to 20 year olds by 5.4%, in specification (e), versus just 0.1% for all age groups. The econometric models were also separately estimated for traffic deaths involving 15–17 and 21–24 year olds. Among the younger group, the only variable (other than the state and time effects) which is ever statistically significant is the unemployment rate. Similar results are obtained for 21–24 year fatalities as for the *TVFR*. In particular, the *MLDA* coefficient is negative and significant in specification (b) but becomes small and insignificant in specification (e), whereas the estimated beer tax effect is only slightly reduced by the addition of controls for alcohol regulations and macroeconomic conditions.

contradictory information on the effectiveness of these policies in reducing traffic fatalities. By highlighting the fragility of the parameter estimates on key alcohol policies to reasonable changes in model specifications, this investigation provides at least a partial explanation for the ambiguous findings.

Special attention is paid to omitted variables biases resulting from the failure to control for simultaneous changes in other types of alcohol legislation and for macroeconomic conditions existing at the time the laws were enacted. For instance, the predicted reduction in the per capita vehicle death rates from raising the minimum drinking age from 18 to 21 declines by 70%, and becomes statistically insignificant, with the addition of covariates for per capita incomes, unemployment rates, and five DUI statutes. In the most fully specified model, dram shop and administrative per se laws are the only regulatory variables ever observed to have a statistically significant negative impact on traffic mortality. Even in these cases, the estimated coefficients are likely to represent upper bounds of the true effects because many of the policies were enacted at the same time as other (not controlled for) legislation and may partially proxy the effects of unmeasured community efforts to combat drunk driving. [31]

This does not imply that all alcohol-control policies are necessarily ineffective. For instance, legal drinking ages are strongly negatively related to the fatalities of 18 to 20 year olds and these estimates appear to be robust to changes in model specifications. Generally, however, future research needs to more carefully control for a comprehensive set of regulatory variables and to account for grassroots activities, such as those by Mothers Against Drunk Driving, if it is to provide useful information on the independent impact of the different types of DUI legislation.

In contrast to the more fragile results for the legislative policies, higher beer taxes appear to reduce vehicle deaths and the parameter estimates obtained from fixed-effect models are relatively insensitive to the choice of specifications. That the omitted variables bias is less severe than for regulations is not surprising when considering that alcohol taxes were *declining*, in real terms, during the period of intensive legislative and social change.

Stricter anti-DUI laws, unless draconian in nature, are unlikely to yield a significant further decline in traffic fatalities. The United States now has a uniform 21-year-old drinking age and most states have instituted stringent policies designed to deter drunk driving. By contrast, substantial decreases in vehicle deaths probably could be obtained by increasing alcohol tax rates, which remain low by historical standards. For instance, the 'preferred' econometric estimates suggest

[31] The policies could, of course, yield benefits other than reductions in traffic deaths. For instance, Cook and Moore (1993b) document a positive relationship between minimum legal drinking ages and levels of educational attainment and Mullahy and Sindelar (1993, 1995) uncover a negative association between problem drinking and future incomes or employment.

that restoring the real 1988 beer tax to the level prevailing 13 years earlier (a 78% increase) would have resulted in a 7 to 8% reduction in highway fatalities, saving 3300 to 3700 lives annually.

Without a full benefit–cost analysis and a further investigation, it is not possible to say whether such a tax increase is desirable. The estimated decline in vehicle fatalities could be overestimated, even in the most fully specified models, if important confounding factors remain uncontrolled for, if the tax elasticities are unusually large during the period of this study, or if beer taxes were increased in isolation (allowing substitution towards cheaper sources of liquor). In addition, any the potential gains associated with light-to-moderate levels of drinking have not been taken into account.

However, the benefits of higher taxes are also certainly understated, to the extent that decreases in drinking and drunk driving confer advantages beyond reductions in crash fatalities. It is also worth noting that only a small portion of any tax increase represents an efficiency cost, with the remainder a transfer of consumer surplus to the government. Furthermore, research by Manning et al. (1989) and Kenkel (1993b) suggests that liquor taxes only partially cover the external costs of drinking and that alcohol problems can more efficiently be reduced by raising taxes than by increasing the legal drinking age.

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